port and a health-resort in one borough, and which, therefore, might be taken into account in any deductions from statistics of health or mortality of their united populations.

British Association, Bristol W. J. BLACK

A Lunar Rainbow?

THERE can be little doubt that your Australian correspondent, Mr. Lefroy (vol. xii. p. 329), has seen one of the phases of an Aurora Australis. Similar appearances have been observed by me in Scotland, passing south of the zenith (and nearly through the anti-dip, as at Fremantle). Their sudden occurrence and temporary persistence are perplexing to those who have not seen this particular display before. The first seen by myself (in 1844, I think) was a single beam which remained in the same position during some hours; it was described by me next day in a local paper, while a well-known observer in a communication to an Edinburgh journal had taken it for a comet.

It is pleasant to see accounts of such phenomena sent to NATURE from all parts of the world, even when the true cause has not always been apparent. It is not improbable that the magnets at Melbourne will have shown some slight disturbance about 8h. 30m. P.M. of May 16.

JOHN ALLEN BROUN

I DO not see any reason to doubt that the phenomenon seen by "J. W. N. L." in Australia, and described by him in vol. xii. p. 329, was an aurora. I never saw one with so many arches as he mentions (eighteen or twenty), but there can be no reason for supposing so large a number to be impossible. In almost every other respect his description agrees exactly with auroras such as may occasionally be seen. T. W. BACKHOUSE

West Hendon House, Sunderland, Sept. 4

The House-Fly

I WAS somewhat interested in Mr. Cole's remarks on the house-fly in NATURE (vol. xii. p. 187), and recently had an example of another of its enemies. On touching a rather small decrepit house-fly which was making its way across a sheet of paper, three minute, active animals, apparently beetles, tumbled out of it; they were light brown in colour, and very much the shape of *aphides*, and about the size of the hole a medium sized pin would make when pushed through paper. F. P.

OUR ASTRONOMICAL COLUMN

M. LEVERRIER'S THEORY OF SATURN.—Early in the year 1874, M. Leverrier presented to the Paris Academy of Sciences the conclusions he had drawn from the comparison of his analytical theory of the planet Jupiter with the meridian observations made at Greenwich and Paris during the long period of 120 years, which he found to be represented thereby with all desirable precision; thus proving that the motion of Jupiter is not subject to any sensible action beyond the effects of the known planets.

The comparison of the theory of Saturn with a similar extended course of normal positions, each one based upon a great number of observations, has not run quite so smoothly, but, on the contrary, has presented some slight difficulties, upon which M. Leverrier makes known his opinion, in a communication to the Paris Academy on the 23rd of last month. During the thirty-two years of modern observations, 1837-69, the differences between theory and calculation, except in two instances, remain below 0·2s. in the times of passage observed on the meridian; for the older observations of Maskelyne and Bradley, somewhat larger discordances are shown. The residuals are, however, upon the whole, very small, and a question arises, whether such quantities can be legitimately neglected, or, if not, whether their cause is to be sought in incompleteness of the analysis or in errors of the observations themselves. M. Leverrier has not been content to rest upon the first supposition, but states that he has used every effort to elucidate the source of the remaining differences. To satisfy himself and astronomers generally that there is no defect or inaccuracy of theory, M. Leverrier has taken extraordinary pains to guard against error or omission. When he found in his earlier researches a discordance between theory and observation in the case of Mercury, he was able to explain the whole by admitting an increase in the motion of the perihelion, which might be attributed to the existence of cosmical matter or the action of small bodies nearer to the sun than the planet; and again, when the comparison of theory with the observations of Mars showed differences, they were explainable by a similar assumption of increased motion of the perihelion, necessitating an increase in the mass of the earth, and consequently of the solar parallax. In the case of Saturn, the smallness of the residuals has rendered it a much more difficult matter to pronounce with confidence upon their cause. Having reviewed the whole of his analytical theory, M. Leverrier, with the view to further verify it, considering this theory as a first, though exact approximation, proceeded by methods of interpolation to reconstruct it, taking account at once of the terms of all orders. Every possible verification having been thus accumulated, he concluded that no error was to be apprehended in this direction. The comparison with the normal positions having been certified with equal care, he ascertained the effect of small changes in the masses of Jupiter and Uranus, the errors being exhibited in functions of the corrections to these masses, and the results prove that no alteration in the adopted value of either mass will destroy the residuals as a whole; if they are somewhat diminished thereby in one part of the series, it is only at the expense of increasing them in other parts. Indeed, M. Leverrier establishes one point, and a very remarkable one it will no doubt be considered, viz., that the 120 years of meridian observations of Saturn are insufficient to afford a reliable value of the mass of Jupiter; or, in other words, that the mass of Jupiter which has so great an importance in the elements of the solar system, is not yet determinable from the comparison of the theory of Saturn with observations. This was not the case as regards the mass of Saturn, which M. Leverrier found from his researches upon the motion of Jupiter to be $\frac{1}{3529{\cdot}56}$ a somewhat smaller value than that resulting from Bessel's measures of the Huygenian satellite.

Under the above circumstances, the probability that errors of observation are the cause of the remaining differences from theory is much increased, and M. Leverrier appears inclined to attribute these errors to the interference of the rings under their various phases, an explanation which practical men will assuredly regard with favour. Considering that at certain times the rings disappear entirely, when the planet's centre may be well observed, while at others intervening in an elliptical form, projecting shadows and occasionally rendering impossible the observation of one of the limbs, there is nothing unlikely, as M. Leverrier remarks, in an uncertainty of some tenths of a second in R.A., which would sufficiently explain all. At any rate, whatever influence the interference of the rings may have upon the observations, it is doubtless of a variable character, as well on account of the physical fact itself, as from the effect it may exercise on personal equations.

MR. DE LA RUE'S TABLES FOR REDUCTION OF SOLAR OBSERVATIONS.—"Auxiliary Tables for determining the angles of position of the Sun's Axis and the Latitude and Longitude of the Earth referred to the Sun's equator," which have been employed in the reduction of the ten-year series of solar photograms taken at the Kew Observatory, have just been printed by Mr. De la Rue, professedly for private circulation, though, as they have been imposed in the size and type of the "Philosophical Trans-

actions," it may possibly be the author's intention to append them to a future communication to the Royal Society, in continuation of other important papers already published in the "Transactions,"—a place which the Tables will advantageously occupy. They give with sun's longitude as argument, the inclination of the solar axis to the circle of declination, reckoned positive when the axis is west of the north point of the sun's disc, and assuming the inclination of his equator to the ecliptic to be 7° 15′·0, and the longitude of its ascending node $74° - \nu$; and with argument, sun's longitude $+ \nu$, the "Heliographical latitude of the earth" and "Reduction of longitude." The obliquity of the ecliptic is taken, 23° 27′·5, but to correct the angle between the circle of declination and the sun's axis, for difference of true and assumed obliquity, a supplementary table is provided.

The Tables have been calculated by Mr. Marth, and it will be obvious to anyone initiated in such work, that considerable trouble has been taken to ensure their accurate production.

MIRA CETI.—A *minimum* of this variable star is set down in Schönfeld's ephemeris for September 30. The minima have not been properly observed nearly so often as the maxima, though equally important in the investigation of the laws which regulate the fluctuations of light, and which, according to Argelander's researches, involve a more complicated formula than has yet been deduced for any other variable. The circumstances of the approaching minimum are very favourable for observation.

SCIENCE IN GERMANY

(*From a German Correspondent.*)

ONLY for a small number of elements and their compounds is the relatively low temperature of the non-luminous gas flame sufficient to produce spectra which can be of use in analytical researches; by far the larger number turn into vapour at such degrees of temperature as we can obtain solely by the electric spark. We are therefore confined to spark spectra for such bodies which do not give spectra in the flame, and these spark spectra can all the less be dispensed with in those cases where *new* elements are sought for, or where it is a question of proving beyond all doubt the presence of certain bodies, which in their chemical properties are so much alike that ordinary reagents do not suffice for their discovery or separation.

But there are difficulties in the way of practically using spark spectra, which have been the reason why these important means of reaction have not yet found their entry into all chemical laboratories. First of all, a simple method has been wanting by which spark spectra can be obtained at any time. Whoever has been obliged to use currents of great intensity with temporary interruptions of days, weeks, or months, knows how much unpleasantness is caused by fitting, taking to pieces, and cleaning the ordinary constant batteries used hitherto. Another difficulty lies in the fact that spectrum tables are still wanting which would be of sufficient service for all practical purposes. It is true that a large quantity of measurements have been published, and doubtless some of them are extremely accurate, but with the greatest part of them the purity of the substances experimented with is not in the least guaranteed, and very often it can be proved not to have been attended to at all. If it is attempted to reduce to a universal scale all the spectrum drawings at hand which have been obtained by different observers, with different refractive media, with different widths of the slit, some at a higher, and some at a lower temperature, tables are obtained which are completely and utterly useless in the laboratory.

Lately Prof. Bunsen, of Heidelberg, has tried to remove all these difficulties. In a very important treatise, the first part of which has just been published, he first describes a new battery and a new spark apparatus, by means of which spark spectra can at any time be obtained with the same ease and facility as ordinary flame spectra. The battery is the charcoal-zinc battery without clay cells. The exciting liquid is a mixture of bichromate of potash and sulphuric acid. In order to prepare 10 litres of this liquid, Prof. Bunsen gives the following instructions:—0·765 kilogrammes of commercial powdered bichromate of potash, which as a rule contains about 3 per cent. of impurities, are mixed with 0·832 litres of sulphuric acid in a stone jar while the mass is being constantly stirred; when the salt is changed to sulphate of potash and chromic acid, 9·2 litres of water are added, the stirring being kept up and the water allowed to flow from a spout about ½ inch wide; the crystal meal, which already is very warm, thus gets warmer and warmer and eventually dissolves completely. The exciters for this liquid are: a rod of the *densest* gas coal, 4 cm. broad, 1·3 cm. thick, and immersed 12 cm. deep into the liquid, and a rolled plate of zinc 4 cm. broad, 0·5 cm. thick, and immersed to the same depth as the coal; the zinc plate is entirely coated with a layer of wax (which is put on whilst hot), except that plane which is turned towards the coal and which is amalgamated. The distance between coal and zinc is entirely optional; in the spectral and analytical researches of Prof. Bunsen it varied according to circumstances between 3 and 10 millimetres. The results with this battery are, however, not very satisfactory with regard to duration and constancy of current, if the cell containing the exciting liquid is made of the same size and shape as those in the ordinary Grove or Bunsen battery. The reason of this lies in the circumstance that in the nitric acid of those batteries there is far more oxygen contained, which is employed for depolarisation, than in an equal weight of the chromate liquid, and that therefore a comparatively much larger quantity of the latter is used up to obtain the same effect. The chromic acid battery therefore, compared to Grove's battery, requires cells of at least three to four times more capacity. The best shape for these cells is that of narrow, high cylinders. The column of liquid, of about 1·6 litres, has a diameter of about 0·088 metres, and stands 0·28 metres high in the cylinder, which bears a mark at that height. The zinc-coal pair is only immersed up to half its height into the liquid column, and has an active zinc surface of about forty-eight square cm.

With regard to the constants of this chromic acid battery *without* clay cells, it considerably surpasses in electromotive force all other apparatus *with* clay cells hitherto used. It possesses an electromotive force which is about 13 per cent. larger than the ordinary charcoal-zinc or Grove battery. Its essential conduction resistance is about 12 per cent. smaller than that of Grove's battery with clay cells. In order to be able to judge the economical effect of the chromic acid battery, we will consider a little more in detail the chemical processes taking place in this battery. In unconnected freshly filled Grove batteries the consumption of zinc is very small, only when after prolonged use an electrolytic and endosmotic exchange has taken place between the two exciting liquids, a consumption of zinc, independent of the generation of the current, becomes apparent. In the unconnected chromic acid battery, however, the consumption of zinc at the very beginning is entirely the same as that which is observed in connected batteries during the generation of the current. This circumstance makes it indispensable to arrange the chromic acid battery in such a manner as to make it easy, at every interruption of the current, to bring the exciting plates out of contact with the liquid. This is attained by a simple hand lever arrangement by which the plates can be dipped into or raised out of the liquid. It is of particular interest, not